

which the warp is pulled in single threads through the sow-box, or vessel in which the sizing liquor is contained, and is afterwards dried by heated air or by passing round cylinders filled with steam. The amount of size in the so-called "pure" cloths varies from 5 to 7 per cent. In such cloths the quantity of fibre is from 92 to 94 per cent., the remainder being made up of mineral matter derived from the raw cotton. Now as one element in determining the value of cloth is its weight, it happened that at about the time of the "cotton famine" which followed the civil war in America, that certain unscrupulous manufacturers introduced the practice of "heavy-sizing"—that is, in plain terms, of substituting cheap mineral substances for cotton.

Some idea of the extent to which this adulteration is practised may be seen from the following analysis of a heavily-sized warp, published by the authors. It will be noticed that only about one-third of the substance is cotton fibre, the remaining two-thirds being made up of clay, flour, and fats, with certain mineral chlorides.

Cotton Fibre	Fibre	33'18	35'83
	Natural moisture	2'65	
Size	Moisture with size	7'81	27'01
	Fats	3'04	
Mineral	Starchy matters	16'16	37'16
	Natural ash	1'00	
	China clay	32'07	
	Chloride of magnesium..	3'25	
	" zinc	0'84	
		100 00	

Very large quantities of a variety of cloth known in the Manchester trade as an "eight-and-a-quarter-pound shirting" find their way to India and China. The general character of a very considerable proportion of this substance may be determined from the following numbers :—

	lbs.	oz.
Warp	2	14
Weft	1	12
	4	10 Pure cloth.
	3	9 Size, &c.
Total	8	3

To the general reader a word or two of explanation concerning the extraordinary complexity of the composition of a piece of modern grey cloth, as revealed by the foregoing analysis, may be desirable. It will be seen that the main weight-giving substance is China clay, which has to be suspended in a sizing liquor of pretty stiff consistency. In order to preserve the clay upon the fibre it is necessary to keep the fabric slightly damp; this is effected by the addition of some highly hygroscopic material to the size, such as the magnesium chloride, which is one of the most deliquescent substances known to the chemist. The constant presence of moisture, however, renders the fabric very liable to mildew, especially if the flour has not been properly fermented before it is incorporated into the sizing liquid; and it is in order to prevent this that some antiseptic is added, usually chloride of zinc.

There is no doubt that in the outset the manufacturers, as a body, set their face against the production of such stuff. Twenty years ago these fabrics had an evil reputation: they were made by tenth-rate manufacturers and

sold by tenth-rate agents. But the heat of competition has changed all this. The immense quantities of these goods which found a market in India and China—indeed, they were mainly made for exportation—compelled the great majority of Lancashire manufacturers to respond to the demand for these combinations of China-clay and starch with a modicum of cotton, a demand which is very largely fostered by the numberless middle men who come between the manufacturer and the consumer. The usual result has followed: the very fact that numbers are engaged in it has given the trade an air of respectability. *Quæ fuerunt vitia, mores sunt.* The other day Mr. Consul Gardner reported from Cheefoo that a bad name attaches to Manchester goods among the Chinese, consequent on attempts "to sell glue as cloth," and it is highly amusing to read how the Manchester Chamber of Commerce waxed indignant, and how they requested Lord Salisbury "to prevent the publication of similar statements in the future"! It is rather significant, too, that whenever a book on the subject of cotton-sizing is put forth, it should be thought necessary by the authors to dwell upon the "moral aspects" of the question in entire obliviousness of the salutary caution that to excuse is too frequently to accuse. Some of the arguments in extenuation would be amusing if they were not grotesque, as in the book before us, where we read, on p. 99, that "no one, we suppose, will deny that for coffin linings, &c., a heavily-sized but cheaper cloth is not just as good as a purer but more expensive article. If this be granted, the existence of such a material is certainly a boon." How very grateful the undertakers ought to feel for such a boon!

It is hardly worth while to take up valuable space by noticing the merits or demerits of a book such as this, the object, or at least the tendency, of which is to show the manufacturer how, by the application of certain scientific facts and principles, he may seek to perpetuate a system which, we honestly think, is simply a gigantic fraud. Our authors comment adversely on the assertion of a certain county court judge, in a case which came before him, that the "warp-sizer and manufacturer, in receiving and giving the order for sizing some warp, had entered into a conspiracy to defraud the public," but it seems to us not improbable that the judge might be perfectly right. It is almost certain that such a system will not be perpetuated: people will not sheathe themselves with shirts of China-clay. The time was when Manchester made cottons for the world, but her supremacy is being rapidly undermined; and who shall say that her sins have not contributed to her downfall?

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

The Intra-Mercurial Planet Question

I HAVE read, in NATURE, vol. xx, p. 597, your editorial on the above subject. To the language of that portion of it relating to my observations I take most decided exception. You have,

unintentionally of course, done me not a little injustice, owing to a misconception of what I have written, and, strangely enough, you have changed my language, giving it not merely a different, but an opposite, meaning.

I regret that I cannot look at all charitably on your baseless charges that I have "made different statements, and exhibited a degree of hesitancy about it." I had thought that my meaning regarding this question could not possibly be misconstrued, but, perhaps, in going over so much ground in so short a letter, I may not have been so clear on every point as I supposed. My desire to divest the subject of all ambiguity, and to defend my observations, if not my character, from the grave charges you have made, is my only excuse for again appearing before the world. Now, if you will give me a little space in your widely-read journal, I will, as briefly as I can, endeavour to make the subject as plain as written language will allow. If in any person's mind there yet lingers the idea that I have made different and contrary statements, my first effort shall be to set him right. Surmising that in one of your charges (different statements) you refer to the estimated distance of 12' between the two objects seen by me during the total phase of the eclipse, I answer by emphatically saying that I have never published such a statement. A little explanation, however, is here necessary for clearness of conception. As soon as I saw the two stars I was confronted with half a dozen questions which required immediate answers, for time was precious, viz.: 1. What stars are they? 2. How far and in what direction from the sun? 3. How far apart? 4. Of what magnitudes? 5. In what direction do they point? What star, in the clearest, darkest night, appears to the naked eye as bright as do these? In response to 3, my *instantaneous* impression was about 12', but, as quickly thinking how wide of the mark I might be in the estimation of so large a distance, I chose to impress it on my mind, knowing that, after arriving at home, I could soon find two stars whose apparent distance would be sensibly the same. This I did, and have several times published to the world the result, viz., that they were a little over half that between Mizar and Alcor, or about 7' apart. What I wrote in my note-book of the 12' I discarded immediately, and all the time have said, in language too plain to be misunderstood, that it was of no value at all. Every published statement has been a reiteration of this, and where, I ask, is the excuse for any who have read my letters and reports to misunderstand this? The distance recorded in my note-book was merely for reference, to see how near the truth the guess would come out. I repeat that I have never published that they were 12' apart, and your charge that I have made "different statements" falls harmlessly to the ground. Have I not adhered with unyielding pertinacity to the facts first published, that they were about 3' south-west of the sun? That they were exactly equal in brightness, and of the fifth magnitude? That the disks were large and red? That they were about 7' apart? And that they pointed towards the sun's centre? In all I have written I have been as guarded as possible, knowing that the time might come when every word would possess a significance not now anticipated. How, then, with any kind of justice, can I be accused and published to the world as having made different and contradictory statements?

Perhaps you base your charge on the mathematical error made in reducing the estimated distance in arc to that of time, in order to show the near agreement in R.A. between Prof. Watson's star and mine, but does that come under the head of "different statements?" If all numerical errors are to be thus classed, who, without sin, can be found to cast the first stone?

I wish it to be distinctly understood that up to this time I supposed (and the fact was disputed by none) that one of my objects was θ Cancri, and the other Watson's planet (α), and I was extremely desirous, while it all was fresh in our minds, to settle the matter, so I wrote to him that I could not harmonise his observations—as published—with my own, though I did not tell him what changes were necessary to attain this result. He replied that after making the necessary corrections, the Dec. comes out + 18° 16', while his previous statement, made before the corrections were applied, declared it to be but 18°. That 16' helped matters very much, but still was only half enough, as the following facts will show. The Dec. of θ is 18° 30' 20", that of the sun at the time of the eclipse was about the same, and, as my two objects ranged with the sun's centre, my new one (his planet (α) as I then supposed) must have had a Dec. almost identical with both, but it is clear that no object with a Dec. of 18° 16' could range with the sun's centre, or anything like

it if one were θ . This is what I meant when, in my reply to Peters, I said, "our difference in Dec. was a source of solicitude to me."

To show that you did not clearly understand the matter you corrected me, inserting in parentheses after Dec. these characters (? R.A.), as though I had made a mistake. No, I made no mistake, but meant just as I said. I had, at that time, but little anxiety about the R.A., supposing that the distance between us was not an irreconcilable one (being ignorant as yet of the error you afterwards pointed out), and this was the way I reasoned. The R.A. of θ was accurately known. I did not, however, know which was θ and which planet (α), but Watson wrote me the planet was nearest the sun, though he located it in R.A. 8h. 27m. 35s., which was too far east to agree with my observation. But I, with great reluctance, increased my estimated distance 1', calling it 8' instead of 7', and, reducing this to time, erroneously called it 2m.; while it was really but 32s. This was as far east, or as near to him, as I could go without doing violence to my better judgment. Here arose the question, "Can I not bring Watson nearer to me?" He said: "I consider my observation trustworthy to within 5' of arc." So I brought him 5' farther west. Nearer to him I could not go, nor nearer to me could I consistently bring him, as he was certain no error had been made. After all, we were too far apart to harmonise things, and, after much reflection, I made another attempt to shorten the bridge over the chasm between us. I tried to imagine that the planet had just passed its inferior conjunction, and, during the five minutes that elapsed between our observations (mine being the later), it had retrograded a little. This was why I contended that it had just passed its inferior conjunction, and that the evidence adduced from their exceptionally large disks was inadequate to prove that it was approaching superior conjunction, when it would, of course, have a very large gibbous disk.

Up to this time the thought that I had seen anything else than θ and Watson's planet (α) had not entered my mind. Being unable to reconcile our difference in R.A., though I then supposed we were not far apart (having as yet no intimation of the above-named error), I turned my attention to the matter of difference of Dec., which I could see no way to reconcile, as it amounted to over 14' as follows:—

$$\begin{array}{r} \text{Dec. + Swift} = 18^{\circ} 30' 25'' \\ \text{" + Watson} = 18^{\circ} 16' 00'' \\ \hline 14' 25'' \end{array}$$

The above Dec., as deduced by me, was published in NATURE, vol. xviii. p. 539, in which I also computed its R.A. to have been (erroneously, as before stated) 8h. 26m. 40s. Commenting on this letter, you pointed out the error of the reduction of the 8' of arc to time. I instantly saw that 8' was but 32s., and that we were really wider apart in R.A. than in Dec. Then I said in reply to Dr. Peters, "The scales fell from my eyes, and I was able to see my way clearly through the mystery, viz., that Watson's planet (α) and θ Cancri were not the objects seen by me."

Up to this point I have endeavoured to make the subject connected and plain, and if I have not then I despair of ever being able to do so.

I now return to your editorial, which, except what you say of myself, is a fair and candid one. Please allow me to quote a few lines from that part of it where you attempt to quote me in my reply to Peters: "He now writes that the difference in Dec. (? R.A.) shown by his own and Watson's observations had been a source of solicitude, and he could see no way to harmonise them till NATURE pointed out the error." &c. I said nothing of the kind, but something as different as the zenith is from the nadir. You, by inserting the characters in parentheses, make me say that I felt solicitude about the R.A. My concern was for the Dec. as I stated it, that of R.A. being nearly wiped out, as I then—unconscious as yet of the aforesaid mistake—supposed.

But the most curious thing of all is that you should interpret me as saying that Watson's and my own observations were harmonised by your detection and pointing out of the error, when just the opposite was the effect. It *dis*harmonised them, for it showed me that instead of our objects being quite near together in R.A., we were more than a half degree apart. This, coupled with our irreconcilable difference in Dec., caused, as I said, "the scales to fall from my eyes," &c. This matter, which at first sight might appear trivial, is a vital one in my defence,

and I wish to make out a perfect vindication, hoping never again to be obliged to recur to it. If you will refer to my reply to Peters, you will see that I speak of our difference in R.A. and stop, coming to a full pause. I then take up the subject of Dec., and when through with that, make another period. Then I say, "Thus the matter rested until NATURE pointed out the error, &c." Is not your language about as unlike this as can well be? In response to your wish to be able to tell your readers "how this sudden illumination caused the scales to fall from my eyes," I hope the above explanation will prove full and clear to all.

Your second charge, "hesitancy about the matter," is a new one, and so at variance with truth that necessity, even at the expense of being prolix, compels me to refute it, and to show to the world that this charge is as baseless as the other. How long did I hesitate? I answer, from the time of the eclipse until just two minutes after my arrival at home, when, though very weary and ill, and before I was seated, I consulted "Webb's Celestial Objects" to see how far Alcor was from Mizar. Then and only then was I able to fix on a definite distance between θ Cancri and, as I then supposed, the planet Vulcan, viz., about $7'$. I left Denver the next morning after the eclipse, coming homeward, both by night and day, as fast as steam could bring me, arriving at home on the P.M. of Saturday, before most of the astronomers had left Denver. I immediately despatched a messenger to the Editor of the *Rochester Sunday Morning Herald*, notifying him of my arrival. I was at once interviewed by him, and a full account was laid before his readers by daylight the next morning. Sunday P.M. I was interviewed by a reporter of the *Rochester Democrat and Chronicle*, which paper, the next morning, contained a long account of my observations, a considerable part of which was published in NATURE. As soon as possible I wrote the facts to the Astronomer-Royal, to the *Observatory*, to Admiral Mouchez, and made out my report to Prof. Colbert, of Chicago (the chief of the party to which I belonged), which, with those of the other members, was published in pamphlet form, also a more extended one to Admiral Rodgers, not yet published. Very little hesitancy in this I think.

I left Denver with Professors Colbert and Hough. On the way Prof. Hough asked me several questions regarding the distance between the two stars. I told him I was unable to give their distance in arc, neither could I think of two stars whose apparent distance was the same. I also said to him that the nearest approach to a resemblance which I could then recall were α^1 and α^2 Capricorni, but, not having observed them with such an object in view, would not say that they were sensibly the same. After they had left me—changing to another road—and before my arrival at Kansas City, and before night of the day of starting, the thought came suddenly to my mind that their distance apart was about equal to a little more than half that between Mizar and Alcor, whatever that might be, which could not be ascertained until my arrival at home.

Since the eclipse I have made many observations of θ Cancri and regions adjacent, to see if my judgment would allow me to modify in any particular my observations as made and published. I have even gone to a part of this city where the streets run parallel with and at right angles to the meridian, as they did at our camp, in Denver, and then wait until an imaginary sun some $30'$ west of δ Cancri had the same altitude and azimuth as had the real sun during totality. And, while I am not inclined to make any changes whatever, I will say that it cannot be denied that, as regards the distance and direction from the sun, they can only be considered as rough guesses, though this does not militate in the least against the existence of the new objects. That they are new I know, for they are not there now. I have never made a more valid observation, nor one more free from doubt regarding the genuineness of the objects seen, which, in my opinion, were circumsolar bodies, unquestionably intra-Mercurial planets. The view of them was as beautiful as it was unexpected, and it was with great reluctance that I could break away from the captivating scene. It must be borne in mind that my telescope was filled with a flood of light, with not an object for reference visible, and therefore, when I ran upon these two round red disks, equally bright, and so near together, it is not surprising that they made an impression upon my mind that never will be effaced.

The great field for future astronomical discovery will, without doubt, be the sun and his immediate surroundings. Let no man's prejudice deter him from taking part in such prospective discoveries, for the field promises rich rewards.

Though I have said above that I am not inclined to modify my published estimations, yet I am willing to say as follows:—If I were compelled to change the brightness of the two stars one magnitude, and say whether they were of the fourth or sixth, I should answer, the former. If I were compelled to change their distance from the sun half a degree, and say whether they were $2\frac{1}{2}''$ or $3\frac{1}{2}''$, I should say the latter. Again, if I were compelled to change their direction from the sun, and say a little farther south or north, I should unhesitatingly say the latter, or, as I said in my report to the Naval Observatory, south of west, instead of south-west. And, finally, were I obliged to change their distance apart, and declare whether they were $6'$ or $8'$, I should, without a moment's hesitation, say the former, or about the distance between α^1 and α^2 Capricorni.

LEWIS SWIFT

Rochester, N.Y., December 10, 1879

The Transverse Propagation of Light

IN NATURE, vol. xxi, p. 256, appeared a paper by Mr. Tolver Preston, on which I wish to make a few remarks.

The author does not make himself very clear as to what he supposes the effect of the vibrating molecules of gross matter on the ether atoms to be. From what I can gather, the effect on a small plane receiving the light from an illuminated "point" would be of the following nature:—When the molecule of gross matter was not vibrating, there would be a more or less shaded spot on the plane, but if the molecule vibrated, then this shaded spot would also vibrate in the same time, which would be possible, since during one vibration of the molecule an extremely large number of ether atoms would impinge on it, and therefore, a large number at each portion of its vibration. In what follows I shall suppose that this is the manner in which the light is supposed to be propagated.

1. The atoms are very small; the free paths are very long. In order that the acceleration of the sun on all the planets must be inversely proportional to the squares of their distances, this mean path must be comparable with the radius of Neptune's orbit; and in order that the light of the stars may be visible, it must be comparable with the distance of the furthest visible star. Again, since, as Mr. Preston says, the automatic adjustment to equality of direction is "of such a rigid character, that if the atoms were imagined to be disturbed or made to move in the most chaotic manner, they would, when left to themselves, instantly correct the irregularity," it follows that the time of describing the mean free path must be very much smaller than the "instantly" small time in which they "correct the irregularity." Their velocity, therefore, must be enormous. They must move to the farthest visible star in a very small fraction of a second. That they have a very large velocity also follows from the smallness of the atoms and the magnitude of gravitation. Now the velocity of light on Mr. Preston's theory must be the velocity with which the atoms move, a velocity which, as has been shown, must be enormously greater than 200,000 miles a second.

2. The above supposes the velocity of all atoms the same, which would not be true. If they varied in the same way as in a gas composed of atoms which do not influence one another, then at a distance from the illuminated point, after a few vibrations of the gross molecule, the shaded spot would not vibrate, but would become an elongated shaded spot without motion, and there would be no light at all.

3. The data of the theory are definite, and it therefore ought to be capable of explaining the laws of refraction and reflection, let alone those of diffraction. This it is incapable of doing; for the light that gets through must be carried by atoms which pass through without striking any of the molecules of gross matter; they must therefore pass through without change of direction or velocity, and therefore cannot be deflected.

These are three reasons, each of which by itself condemns the ingenious explanation offered by Mr. Preston.

W. M. HICKS

St. John's College, Cambridge, January 16

Mountain Ranges

It is to be regretted that Mr. Trelawney W. Saunders should make confusion worse confounded by noticing imaginary discrepancies based upon a mistaken assumption of a natural agreement. In his paper "On the Mountains of the Northern and Western Frontier of India," published in NATURE, vol. xxi, p.